As all of these calculations are based upon relatively short records, it did not seem wise to give a false impression of accuracy by carrying the final probability values to many decimals, and the average intervals are given to the nearest whole year.

Column 6 of this table shows that severe outbreaks of *P. orthogonia* may be expected about once in 20 years in central Montana and Wyoming, about once in 30 years in eastern Montana and Colorado, and about once in 50 years in North Dakota. The interval of 250 years at Moorhead places this station definitely outside the economic distribution of this insect. It is, of course, possible that one extremely favorable year following one with a rainfall btween 4 and 5 inches may cause a local outbreak of moderate severity, and such local outbreaks may be expected somewhat more frequently. There are indications, not climatic in character, that a small district in southern Alberta and extreme north-central Montana may suffer more frequent outbreaks than any other region studied, but no long rainfall records are available for a climatic study.

The comparison of actual and expected pairs of dry years shows a fairly close fit of theory to observation. The wide deviation in the case of Helena is explained above, while in the case of Crow Agency there are several gaps in the record which might have included one or more dry years.

CONCLUSIONS

This study has shown that there is a close connection between the chances of a single favorable year and the ability of *Porosagrotis orthogonia* to maintain itself at a place. At least 1 year in 10 must be favorable to increase or the insect will disappear. Outbreaks brought on by migrating moths are confined to regions where at least 1 year in 20 is favorable.

There is a decided tendency for like years to follow in succession, and this tendency was evaluated, without any implication of periodicity, by forming a distribution curve of interannual differences, neglecting signs. In all cases a decidedly skewed distribution was obtained from which the probability of a deviation of 1 inch or less between successive years was calculated.

Severe attacks of the pale western cutworm may be expected only at long intervals in most parts of its range. In the dry mountain foothills near Helena such outbreaks will be about 16 years apart, on the central plains of Montana and in Wyoming it may occur once in 20 to 25 years, and in eastern Montana and Colorado the average interval between outbreaks is about 30 years. In other parts of its range in the United States it will probably be more rarely injurious. Light outbreaks may be expected somewhat more frequently.

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ON THE MEASURE OF CORRELATION: A REJOINDER

By GILBERT T. WALKER

The note upon my article on the above subject by Mr. Edgar W. Woolard in the MONTHLY WEATHER REVIEW of October last, raises several points of interest, and I hope I may be allowed to make some comments.

1. The fundamental issue can best be seen in a numerical example. The longest series of forecasts known to me is that based on a formula of 1908 for monsoon forecasting in India. The departures of monsoon rainfall given on the 1st of June of the years 1909–1927 by the regression equation, i. e., the amounts determined by external factors, were:

1909	+1.4	1919	−0.7
1910	+1.3	1920	−2.0
1911	+0.7	1921	+2.6
1912	+2.1	1922	−0.2
1913	−0.6	1923	+1.5
1914	+0.7	1924	+0.5
1915	−1.0	1925	+0.8
1916	+4.5	1926	+1.1
1917	+2.9	1927	+1.4
1918	+0.2		

The actual departures during these years were:

1909	+1.9	1919	+3.2
1910	+2.0	1920	−4.3
1911	−3.2	1921	+1.4
1912	−1.1	1922	+2.0
1913	−1.7	1923	+1.5
1914	+3.4	1924	+3.1
1915	−3.0	1925	−1.6
1916	+5.0	1926	+3.6
1917	+7.1	1927	−1.6
1918	−6.5		

2. The S. D. of the first series is 1.73, and of the second is 3.44, the ratio of the first S. D. to the second being 0.50; while we find the correlation coefficient between the two series is 0.56. Now r^2 is 0.31, and I contend that in the long run the ratio of the first series to the second will be r, not r^2. This fraction must, in the long run, be the same in whatever reasonable way the two series are compared, for they obey the same distribution law. When it is said that I take the S. D. as the measure of variation, while Krichewsky takes the square of the S. D., the statement is, in effect, precisely equivalent to saying that I compare the actual series while Krichewsky compares their squares. I hold that if we compare 1, 2, 3, 4, 5, . . . with 2, 4, 6, 8, 10, . . . the first series is half the second and that we can not justify calling it a quarter (or an eighth) by saying that the square (or the cube) of the terms is considered.

3. I would like now to comment on some of the arguments used. When I state that a fraction $r\sigma_0$ of σ_0 is due to variations in x_1, I do not attribute "the remainder $(1-r)\ \sigma_0$ to variations in x_2, . . ." (p. 460). For if $x_0=p+q$, where p and q are independent, it is easily seen that the S. D.'s σ_p, σ_q, of p, q, satisfy the equation $\sigma_p^2+\sigma_q^2=\sigma_0^2$, not $\sigma_p+\sigma_q=\sigma_0$; so the S. D. of the remainder here must be $\sigma_0(1-r^2)^{\frac{1}{2}}$.

On p. 461 there is a determination of the average value of the term $r\sigma_0 \frac{x_1}{\sigma_1}$, which is accepted as the contribution from x_1. Now I gave in paragraph 4 on p. 460

what claims to be a mathematical demonstration that the standard deviation is $r\sigma_0$: It seems therefor rather doubtful whether the conditional argument "*If*, as frequently happens, B′ is practically zero, then" Dines' Theorem would hold, can be said "to dispose of Walker's objection to Dines' Theorem."

DISCUSSION

There seems to me to be, in reality, no conflict between the ideas of Sir Gilbert Walker and those which I tried to express in my note to which he refers: That the ratio of the S. D.'s of the two series given by Walker should be r (if, as Walker assumes, b is independent of x_1) is, it will be found, stated in my note; it is also clear from my equation (2) that the mean of x_0/σ_0 will be r times the mean of x_1/σ_1. Any argument as to whether we should use the S. D. or its square as a measure or index of variation is futile; logically, we are free to use any measure we please, though in practise a particular one may be much more convenient for some purposes than any other; different measures will result in different theorems, but these can not be inconsistent, nor, as Walker rightly insists, alter any facts—nor will they, if strict attention be given to the adopted *meaning* of the terms used. For *certain purposes*, a discussion of which has not entered into these notes, Krichewsky found the square of the S. D. more convenient than the S. D. itself.

The theorem given by Walker in the last sentence of the first paragraph of section 3 of his note above is likewise explicitly accepted in my note; but the inconsistency implied by Walker does not exist, because the remainder of the S. D. is not the same thing as the S. D. of the remainder, and

$$\sigma_0 \equiv r\sigma_0 + (1-r)\sigma_0 = \sqrt{(r\sigma_0)^2 + [\sigma_0(1-r^2)^{1/2}]^2}.$$

In the second paragraph of section 3, Walker, by overlooking some essential phrases and italicized words, changes the intended sense of my statements to which he refers, and fails to reproduce the point I tried to make.

As F. J. W. Whipple observes in a recent note on this subject (Meteorological Magazine, **63**, 12–14, 1928), the difficulty is in establishing that certain enunciated rules are equivalent to certain given equations. It was my principal object to establish several equations, which together result in several consistent theorems relating to different aspects of the question under discussion; which of these equations or theorems is most useful in appraising the value of a correlation coefficient doubtless depends on the purpose for which the coefficient is to be used.—*Edgar W. Woolard.*

NOTE

A copy of the above discussion was submitted to Sir Gilbert who replies as follows:

"I am sorry to learn from Mr. Woolard's remarks that I have at times failed to catch the meaning that he intended to convey, and glad that there is no fundamental difference between us.—*Gilbert Walker.*"

METEOROLOGICAL SUMMARY FOR SOUTHERN SOUTH AMERICA, FEBRUARY, 1928

By J. BUSTOS NAVARRETE

[Observatorio del Salto, Santiago, Chile]

During February the atmospheric circulation continued to show very moderate intensity; however, storms of some importance were beginning to appear over the southern region.

Two cyclonic storms are to be mentioned as important: That of the 13th–16th, which crossed the far southern region and was accompanied by generally foul weather in the southern zone with heavy winds and rain north to the coast of Arauco; and that of the 27th–29th, which passed over the region visited by the earlier storm and likewise brought unsettled weather and rain.

At Valdivia, which is one of the rainiest points on the western coast of South America, the total monthly precipitation was 3.84 inches [normal 2.80 inches—*Translator*] and the maximum amount in 24 hours, 2.16 inches on the 15th.

The anticyclones causing the periods of fine, settled weather were charted through the following periods: 2d to 10th, 9th to 12th, 16th to 20th, and 20th to 26th. The second HIGH remained stationary over Chiloe; the others moved from Chiloe toward northern Argentina.

In general temperatures were moderate in the central zone of Chile, but about the 22d there was a period of very warm weather with maximum temperatures around 91°–92° F. On the central coast there was considerable cloudiness and frequent occurrences of early morning fog.—*Transl. by W. W. R.*